

XXIV. *Experiments and Observations on the inflammable Air breathed by various Animals. By the Abbé Fontana, Director of the Cabinet of Natural History belonging to his Royal Highness the Grand Duke of Tuscany; communicated by John Paradise, Esq. F. R. S.*

Read March 11, 1779.

PHILOSOPHERS believed, till lately, that inflammable air had the power of killing animals who breathed it. Dr. PRIESTLEY, to whom we are much indebted for many discoveries and observations relative to inflammable air, made in consequence of Mr. CAVENDISH's excellent paper on that subject, assures us, that inflammable air causes the death of animals as readily as fixed air, and that animals die convulsed in it. The doctor adds, that water absorbed about one quarter of the inflammable air shaken in it, after which a mouse lived in it as long as it would have lived in an equal quantity of common air. This air breathed by the mouse was still inflammable, though not so much as before.

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Mr. SHEELE, who has made various important observations in chemistry, on the contrary asserts, that inflammable air not only does not kill the animals who breathe it, but that it is even good and innocent air. He relates some experiments to which it seems that nothing can be opposed, and they appear to contradict Dr. PRIESTLEY's observations. Mr. SHEELE has breathed inflammable air contained in a bladder, without receiving any hurt.

Seeing then that the experiments of these celebrated persons contradicted each other, I began to suspect that they might possibly be all true; and that their so contradictory effects might be owing to some circumstance not yet attended to.

In order to follow some method in my researches about a point so delicate, and which so nearly interests human life, I first of all thought of assuring myself, whether or no animals could breathe inflammable air with impunity, when the receivers that contained it were immersed in quicksilver. To this end, I introduced inflammable air, extracted both from zinc and iron, by means of the vitriolic acid, into various tubes filled with quicksilver, in which the air entered pretty free from moisture. I then introduced various birds into those tubes, and observed that they died in a few minutes

time, but without any apparent sign of convulsions. These experiments, having been often repeated, were constantly attended with the same event.

Being assured, beyond any doubt, that the inflammable air obtained from zinc or iron, and made to pass through quicksilver, was fatal to animals; I next wished to observe, whether it retained the same properties when it had passed through water; in which case the volatile sulphurous acid, or other vapour, is absorbed by the water; but, on trying the experiments, I found that the birds died under these circumstances as under the others (though not quite so soon) shewing likewise some signs of convulsion. I introduced some of this same air that had passed through water into a glass tube full of quicksilver, by a method which makes the air lose all its moisture. The birds died in it in the same manner as when the experiment was tried upon water. In all these cases the air after the animals had died in it was still inflammable, nor did its exploding properties seem to have been at all diminished.

The inflammable air extracted from zinc, and that extracted from iron, is fatal to animals even after it has been shaken in water for a minute's time, or something longer. By shaking it a long time, it becomes in some measure respirable; but then it is decomposed in a great measure,

measure, and becomes of another kind, although it still preserves the properties of being inflammable, but in a smaller degree,

Not only birds but also quadrupeds die in inflammable air (though not so soon) and shew some signs of being convulsed.

It seems very strange, that Mr. SHEELE could breathe inflammable air with impunity, when animals obliged to breathe it were killed in a very short time. Admitting his experiments to be true, there remains nothing to be said, but that the inflammable air in which animals die does not occasion death because it is conveyed to the lungs, but because it affects some other organs of the animal body exposed to that air, and necessary to animal life. It is not impossible to occasion death by affecting the very sensible nerves of the nose; it being well known, that various liquors, as very concentrated volatile alkaly, &c. if they are inspired through the nose, immediately affect the senses, and occasion death if they continue to act upon the *pituitary* membrane.

In order, therefore, to try whether inflammable air killed, only because it was inspired through the nose, I stopped very accurately the noses of various birds with soft wax, and in this manner I introduced them into
receivers

receivers full of inflammable air extracted from zinc, and from iron, through water. The birds died within a few seconds, that is, just as they did when their noses were unstopped. Quadrupeds were tried after the same manner, and the event was the same.

Having in this manner exploded this new hypothesis, there remained one more, which seemed to suggest a probable reason (since some reason there must be) for Mr. SHEELE's experiments being attended with results so different from those of other experimenters. When an animal is introduced into a vessel of inflammable air, its whole body is exposed to that air; and it is not yet known by philosophers what disorders that fluid may occasion to the animal frame. It is true that none are observed to be produced by other noxious kind of air; but if it be considered, that the vapours of sulphur make a great impression upon frogs, even when those animals do not breathe them, but have their *aspera arteria* tied up, it will not seem impossible for the inflammable air, in some manner or other, to act upon the body of animals. It may, perhaps, hinder the perspiration; it may insinuate itself through the pores of the skin; in short, its action upon the body seems probable till experiments evince the contrary.

I therefore endeavoured to force various four-footed animals to breathe the inflammable air through the mouth only, without immersing their whole bodies into it. I chiefly used bladders tied to their mouths, but sometimes I also made use of tubes which entered immediately into the wind-pipe. In both cases the animals died in a very short time: hence it became evident, not only that the inflammable air is pernicious to animal life, but that it does not act on the *body* of an animal; for I kept some of them immersed in inflammable air, with the mouth only out of it, and did not perceive any effect hurtful to them.

It being in this manner ascertained, that the inflammable air could not be breathed by animals with impunity, it still remained to find out the cause of Mr. SHEELE's mistake. I began therefore to breathe the inflammable air contained in bladders, after the manner of Mr. SHEELE. The inflammable air used in my experiments was extracted from zinc and from iron by the action of the vitriolic acid, and it was received into bladders that were dry in the inside, but a little moist on the outside. The quantity of air contained in each bladder was about eighty cubic inches. The air coming out of the matrafs passed through about one inch of water before it went into the bladders. At first I
breathed

breathed the inflammable air with a kind of fear; but finding that it occasioned no painful impression, I continued breathing it with courage as long as I could. I breathed in a bladder filled with it eleven times, beginning after a natural expiration. This air when taken out of the bladder, was still inflammable, and being tried with the test of nitrous air it gave II-28, III+20.

Before I go farther I must explain the formula which I use to express the diminution of respirable air, or air of other kind, when mixed with nitrous air. My method is as follows: I have a glass tube of about eighteen inches in length, and half an inch in diameter, closed at one end, and of a constant diameter throughout its whole length: this tube has a mark at every three inches, which marks or divisions I call *measures*, and every inch is divided into twenty equal parts; so that every *measure* is divided into sixty portions, which I call *parts*. Into this tube, by means of an instrument which measures always one constant quantity of air equal to one measure of the tube, I introduce two measures of respirable air and one measure of nitrous air, after which I measure the diminution; then I introduce a second measure of nitrous air, and again measure the diminution. The whole measures I express in Roman characters, and the

parts of a measure I exprefs in common numbers; for instance, when I fay $11-16$ and $11+10$, the first expreffion means, that after having introduced into the tube two measures of common air, and one measure of nitrous air, the space occupied by the mixture of these two airs was two measures $- 16$ parts, or 60ths of a measure: and the second expreffion shews that, after having introduced another measure of nitrous air, the space occupied was two measures $+ 10$ parts. The reason and particulars of this method will be given hereafter in a paper expressly written upon the method of determining the degree of the salubrity of the air by means of nitrous and inflammable air.

Having introduced eight cubic inches of common air into the same bladder, I breathed it as long as I could; beginning after a natural expiration as in the experiment above related. I breathed it thirty-four times successively, and afterwards found it very much altered, so that it extinguished a light many times successively. An animal introduced into a vessel of that air immediately gave signs of uneasiness: and the air being tried with the nitrous air gave $11+20$, $11+15$; whereas, before it had been breathed, it gave with the same nitrous air $11-15$, $11+18$.

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This experiment shews, that the air which remained in the bladder in the first experiment was not so good as that breathed thirty-four times successively. In order to make this experiment with more precision, I breathed eighty cubic inches of common air, introduced into the same bladder, only eleven times; beginning after a natural expiration. Then I examined this air with the nitrous air, and found that it gave II-13, III+28.

Hence it is plain, that the mixture of inflammable and pulmonary air breathed eleven times is much inferior to common air breathed an equal number of times; so that there can remain no doubt but that inflammable air is at least worse than common air.

Willing, however, to ascertain this matter still better, I tried to breathe it immediately through a large receiver, partly immersed in water, and swimming in it, so that the air within the receiver was of the same elasticity with the external air. For this experiment I made use also of a glass tube bended in two different directions.

The air contained in the receiver was about 250 cubic inches. In all the trials made in this manner, I was never able to breathe the inflammable air more than three times, and even at the second inspiration I felt a great oppression. As these experiments can be depended upon, because they were often and at different times repeated,
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there seems to be reason enough to suspect, that the bladder might possibly alter the nature of inflammable air, and render it more fit for respiration, notwithstanding that the mere contact of the bladder seemed not sufficient to produce such an effect, which is irreconcilable with other facts: yet some reason must certainly exist sufficient to explain Mr. SHEELE's experiments, which directly prove that the inflammable air contained in bladders can be breathed with impunity.

When I breathed this air according to Mr. SHEELE's manner eleven times successively, I not only breathed it without any inconvenience, but observed that the first inspirations were even pleasing; more so than when I breathed common air. I felt a facility of dilating the breast, as if the air was as light as that at the top of high mountains. I never felt a like sensation, even when I have breathed the purest dephlogisticated air. I do not think that I was mistaken in these sensations, or gave a loose to imagination, because I was rather prejudiced against the inflammable air, after I had seen various animals immediately die in it, and I was rather fearful when I first began to breathe it: besides, this facility of breathing it, accompanied with a pleasing sensation, I have constantly observed in all my experiments upon this subject.

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This pleasure, however, I paid very dear for in another experiment, in which I was near losing my life. Having filled a bladder of the largest sort with about 350 cubic inches of inflammable air extracted from iron filings through water, which air was not at all diminished by the mixture of nitrous air; I began to breathe it boldly (owing to the encouragement received from the above related experiment), and resolved to breathe it as long as my strength would permit me, after having made a very violent expiration in order to evacuate the lungs of the atmospheric air. Having made the first inspiration I felt a great oppression upon my lungs. Towards the middle of the second inspiration I heard Mr. CAVALLO, who favoured me with his assistance in these experiments, say, that I was become very pale: by this time the objects appeared confused to my eyes. Notwithstanding this, I made the third inspiration; but now my strength failing, I lost my sight intirely, and fell upon my knees. In this situation I breathed the air of the room, but my knees not being able to support me, I fell intirely upon the floor. However, in a short time I came to myself, so as to be able to get up, &c.; but my respiration continued to be affected with difficulty and pain, as if I had a great weight upon the breast; nor did I perfectly recover before the next day.

It must be observed, that during this experiment I kept my nose close stopped.

This same inflammable air contained in the bladder, which I had breathed three times, was examined in various manners, and was found to be as inflammable as before; it exploded as usual, when mixed with dephlogisticated air, but after having been shaken in water for a short time, being tried with the nitrous air, it gave III-10, IV-10, whereas before it was not at all diminished. At this time the common air, with the same nitrous air, gave II-14, II+10. Hence it appears, that the inflammable air, after being breathed, is rather better than before, because in that case it is a little diminished by the addition of nitrous air.

In order to ascertain whether this alteration was occasioned by the bladder or no, I made the following experiment, which, having been often repeated, was constantly attended with the same event. I introduced into a bladder, which was sometimes moist and sometimes dry, a quantity of inflammable air, extracted as well from zinc as from iron, through water, and having kept it in that situation for some minutes, beating in the meanwhile the bladder, to keep the air in agitation, I afterwards took it out, and by the mixture of nitrous air ob-

served, that it suffered no diminution, exactly as it suffered none before it had been put into the bladder.

Having ascertained, in this manner, that the bladders do not in any manner contribute to render the inflammable air extracted from metals better in its nature, there remained no other way of ascertaining Mr. SHEELE's experiments, and of understanding why I had been able to breathe it eleven times, than by supposing that the air of the lungs, which can never be thoroughly emptied by being mixed with the inflammable air, alters it, &c. It is well known, that in an ordinary expiration about thirty-five cubic inches of air are expelled from the lungs. In a very violent expiration, following a natural inspiration, the air expelled may amount to sixty cubic inches. These forty inches of pulmonary air are mixed with the inflammable air, and are expelled from the lungs in proportion to the remaining air that is breathed after that the lungs have been thoroughly emptied. In the experiment above related, of the three inspirations I made into the inflammable air, it may be easily supposed, that twenty inches or more of pulmonary air were joined with the inflammable air, and entered into the bladder. This pulmonary air, although it is itself partly phlogificated, is however diminished by nitrous air; and when it stands in the bladder it is nearly equal to $\frac{1}{17}$ th of the inflammable

air of the bladder breathed three times; hence this loft ten parts by the mixture of nitrous air.

This explanation, which it is necessary to adopt after having exploded all the other hypotheses, is very analogous to the above related experiment of the smaller bladder filled with inflammable air which was breathed eleven times successively. This air was breathed after a natural expiration, so that there still remained in the lungs about seventy-five inches of common air. These seventy-five inches of pulmonary air, together with the eighty inches of inflammable air, were mixed together during the eleven inspirations and expirations; hence the air of the bladder was a mixture of nearly equal portions of inflammable and common air; and, accordingly, when tried with the nitrous air, it was found to be much better (though it had been breathed eleven times) than the air of the large bladder breathed three times only, after the lungs had been emptied as much as possible.

All the other experiments that I have made in confirmation of this hypothesis seem universally to favour it. If a Guinea pig is introduced into a receiver containing 400 cubic inches of inflammable air, or a small bird into only fifty inches of it, and they be left therein till they are dead, that air afterwards will not be sensibly diminished

nished by nitrous air; but if a much larger animal is introduced into the 400 inches of inflammable air, or a small animal into a few cubic inches of that air, then it will be found to be sensibly diminished by nitrous air; and this diminution will be greater as the animal is larger in proportion to the quantity of inflammable air. A larger animal imparts a greater quantity of its pulmonary air to the inflammable air; and the inflammable air will be found joined to a quantity of pulmonary air, which is so much the less as the animal is smaller.

Mr. SHEELE says, he found that the inflammable air after being breathed some time intirely loses its inflammability; from whence he concludes, that the lungs, instead of imparting some phlogiston to, imbibe it from, whatever substance it can be extracted. Though all the direct experiments which shew that a phlogistic principle is continually detached from the lungs, and joins itself to the common air, were wanting, still Mr. SHEELE's consequence could not be drawn, because the experiment is not true. With respect to my own experience I may safely say, that I have always found it inflammable in every circumstance, even after I had breathed it eleven times successively: and I have not only found it inflammable in the bladder, but I have fired it in the act of let-

ting it out of my mouth. In this manner a flame may be produced from the mouth, of various inches in length, and two or more inches in breadth.

But whence does that sensation of levity and facility of breathing the inflammable air, which I have described above, originate? At present I can only have recourse to a mere mechanical cause for a solution, for I do not observe in inflammable air any property that seems capable of altering the lungs upon a chemical principle; neither have I observed any decomposition of air, or alteration of the fluids of the animal. It has been observed, that inflammable air, after being breathed, comes out of the lungs with the same properties it had before. It is also known, that inflammable air is not sensibly absorbed by water, at least after a short time. The lungs, or more properly the pulmonary vessels, are continually moistened with fluids; but that air cannot be absorbed by them, except it be first decomposed. Nothing else therefore remains to which we can have recourse for an explanation of the above mentioned sensations, but the well known levity of the inflammable air compared with common air. And indeed the sensation I felt when I breathed that air, is like that of a very light fluid which does not oppress the lungs, and is hardly felt. This explanation agrees exactly with
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some experiments I have made with common air rendered more light by fire. This air I have found may be breathed much easier, although not for so long a time, as when it is more condensed. It must be said, indeed, that this is occasioned by another particular cause, which has nothing to do with the case of the inflammable air, and which cannot be properly examined in this place.

After all, it still remains to be known, why inflammable air, which kills animals so soon, may be breathed without any oppression, when in a small quantity, *viz.* when it is mixed with common air; and the following experiments, which are very analogous to those related above, will shew that the question is not uninteresting.

I introduced 350 cubic inches of common air into a bladder, and after having made as strong an expiration as I could, I applied the neck of the bladder to my mouth, and breathed the air it contained forty times successively. Afterwards, having taken the air out of the bladder, I found that it extinguished a light several times successively. It formed various crystals with the oil of tartar, but after a very considerable time; some of these crystals had the shape of needles, others were like flowers: being tried with the nitrous air, it gave II-18, III+18. This air, therefore, was very much phlogisticated, nor could I possibly have breathed it longer than I did, with-

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out falling upon the ground, as I already felt my strength failing, and the objects appeared confused before my eyes. Into ten cubic inches of this air I introduced a small bird, which, as soon as it began to breathe it, made various contortions with its body, and seemed to suffer a great deal. It died in ten minutes time; whereas another little bird introduced into a like quantity, that is, into ten cubic inches of common air, lived in it fifty-two minutes, nor did it shew any sign of uneasiness before it had been in five minutes.

It remains to be accounted for, why the bird could breathe for five minutes longer in the air of the bladder than a man could. It will be sufficient to consider, that when a man in this experiment has made the last expiration into the bladder, he is in a state of pain, and his lungs are loaden with a superfluous quantity of phlogiston, which is not communicated to the air of the bladder; whereas nothing of this takes place with the bird, which, besides its being in vigour, has a quantity of common air in its lungs. This seems confirmed by an experiment, which admits of no doubt. Having breathed the air of the bladder as long as I could, I stopped the neck of the bladder with my finger, then breathed the common air several times; and afterwards putting the neck of the
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same bladder to my mouth again, I breathed that very same air four times successively. Now there is no doubt but that a bird could have breathed it much longer: the reason of which diversity seems to be the following, *viz*: that a small bird is in want of a small quantity of air for every time it breathes, whereas a man is in want of a much greater quantity; hence the air is rendered more easily noxious, and unfit for respiration. From all which it may be concluded, that we are in want of a certain quantity of common air necessary for respiration, and for the support of life; and that this air, after being inspired, comes out of the lungs less fit to be breathed a second time.

It has been observed, that the inflammable air cannot be breathed when the lungs are emptied of common air as much as possible; but that it may be breathed when the lungs are in a natural state, in which state a quantity of common air, equal to about forty cubic inches, is known to exist in the lungs of an adult person. This pulmonary air is not infected so far as to be incapable of being breathed various times, and of supporting life. After having made a natural expiration I have with force expelled from my lungs about thirty inches of air into an empty bladder; and this pulmonary air I have generally been able to breathe eight times successively,

but never longer. It is true, however, that I breathed it with some oppression, even from the beginning, which does not happen when the inflammable air contained in a bladder is breathed, the lungs being in a natural state.

And now it seems no longer difficult to give an answer to the question proposed above, and to account for the small difference observed in the breathing of the two different kinds of air in the bladders. The inflammable air, when joined to a great quantity of common air, may be breathed safely, because there is a quantity of common air sufficient for various inspirations, and that the mixture of the two airs may be breathed till this common air is thoroughly infected. But the inflammable air itself is not altered nor decomposed by the respiration. Wherefore we must conclude, that the inflammable air is not such a kind of air as can by itself alone be directly useful for respiration. It must rather be considered as if there was nothing of that air in the case of the bladder; and indeed it is found by experience, that the pulmonary air itself may be breathed eight or nine times in an empty bladder. The not being able to breathe it eleven times successively, as was done when there was inflammable air in the bladder, and the feeling an oppression in the first case and not in the second, must be intirely attributed to the want of thirty-five cubic inches of air expired,

expired, which are necessary to give the lungs all the necessary expansion; whereas, in the other case, the inflammable air serves to fill up space, and, together with the common air, contributes to fill the lungs; so that the inflammable air, considered under these circumstances, and under this point of view, may be said to be useful for animal respiration. This explanation seems most evidently demonstrated by the following experiment. If thirty-five cubic inches of common air are introduced into the bladder, and this air be breathed when the lungs are in a natural state, it will be found, that one may breathe it for twenty times or longer; whereas, when the bladder was empty, it could not be breathed more than nine times at most.

Before I finish this paper it will be proper to mention another cause, which, perhaps, also contributes to render the inflammable air of the bladder less noxious: this is the levity of the inflammable air itself with respect to common air, which hinders the inflammable mixing with the common air. The inflammable air swims continually upon the common air, just as æther swims upon water; and the inflammable air swims still better than æther, because it is much lighter in comparison than æther. Various experiments made upon volatile substances have convinced me of this truth. If equal quan-

tities of common and inflammable air, or dephlogistigated and inflammable air, are put into a tube, and two birds are introduced in it, so that one of them may stand at the top, and the other at the lower part of the inverted jar; it will be found, that the first mentioned of these birds not only will die considerably sooner than the other, but will shew signs of uneasiness as soon as it is come to that place.

The inflammable air, therefore, when breathed together with a considerable quantity of common air, must always swim at the top of it, filling the cavity of the wind-pipe, &c. while the common air occupies the lower place, and filling the smallest pulmonary vesicles is subservient to the ordinary functions of the lungs.

Here I put an end to my observations upon inflammable air considered with respect to respiration; but I beg leave to add a few words respecting a property of the inflammable air, which, as far as I know, has been overlooked by the most diligent observers.

I mean here to speak of such inflammable air as is extracted from metals, by means of oil of vitriol, especially that extracted from iron and zinc. The air of these metals, when presented to the flame of a candle, not only burns with a whitish flame inclining to green (as is well known); but exhibits a kind of sparks or explosions
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which may be easily distinguished between the body of the flame by their vivid light. These sparks, which are of a vivid colour, dart in every direction. They might be easily taken for those sparks that are emitted from red-hot iron; or they might be compared to very small grains of gunpowder, if these were inflamed successively, and without smoke; or they might even be compared to charcoal that sparkles, but without any noise. This phenomenon seems very interesting, as it respects the nature of the inflammable air itself. What seems to me most singular is, that this appearance forms a distinctive character between the inflammable air of metals, and that extracted from animal or vegetable substances; at least I may safely say, that I never found the inflammable air of animal or vegetable substances sparkle like that extracted from metals. In several of the former kinds of air I could observe no sparkling at all; in others the sparks were so few that they might be considered as nothing in comparison to the sparkling of the inflammable air from metals.

The inflammable air of metals itself, if left in contact with water for a long time, or shook in it till it becomes less inflammable, will in great measure lose its sparkling property, and at last loses it entirely, when it is become in a state of being hardly inflammable. I have observed,

that the inflammable air is the more difficult to be decomposed, by being shaken in water, as the number of the sparks it shews when burning is greater; and according to this number of sparks, the inflammable air makes greater or weaker explosions when mixed with the dephlogisticated air; so that it seems proved by experiments, that the phlogistic principle is more fixed and in greater quantity combined with the inflammable air of metals, than with that of vegetable or animal substances. I do not mean to deny the possibility of finding other species of inflammable air extracted from other substances besides metals which may explode like that extracted from metals; but I only say, that in those cases the inflammable air will also sparkle more, and will be found less easy to be decomposed by water. There are other substances that give the inflammable air in great quantity, and which cannot be considered as animal or vegetable substances, but come rather near the nature of metals; as, for instance, the spathose iron, from which I extract a good deal of inflammable air by the action of fire only, applied to a matraass. But the metal in this substance is not in its pure state, and it may be considered rather as a calx of iron than true iron. Accordingly, this air can hardly sparkle at all; it explodes more like
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the inflammable air of vegetable or animal bodies than that of metals, and it is easily decomposed in water.

This property of inflammable air of metals which I have discovered, throws great light upon the analysis of the decomposition of that air which I have made in two different ways. The first is to fire it together with common or dephlogisticated air, in vessels filled with very pure quicksilver, and also in vessels filled with distilled water. The second method is to decompose it by shaking it in pure distilled water. In the first process a great number of experiments are required in order to obtain a sensible residuum; besides, the igneous part is lost. The second method requires an exceedingly long time, but it is the most complete; for which reason I have used it for the decomposition of other kinds of air.

